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A NOTE.

The following is an extract from a letter received by the Author from the late Sir Joseph Hooker:—

“I am much obliged to you for sending me your pamphlet on Herbert Spencer’s refutations, to which I cordially subscribe. Spencer owes little, if anything, to me beyond a cordial encouragement in his botanical studies, and perhaps a few suggestions and specimens from Kew to experiment with.”

HERBERT SPENCER

REFUTES

RECENT MISREPRESENTATIONS.

PROFESSOR BOURNE'S DEFAMATORY
ATTACKS MET BY EXCERPTS CULLED
FROM THE PHILOSOPHER'S WORKS.

BY
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CONTENTS.

	PAGE
Introduction	I
Herbert Spencer, Hooker, and Huxley	5
Original Biological Investigations	11
Conclusion	14



INTRODUCTION.

The Herbert Spencer Lectureship was founded by Mr. M. Shyamaji Krishnavarma, who, in accordance with an announcement which he made at Spencer's cremation on the 14th December, 1903, presented £1,000 to the University of Oxford for the purpose. Lectures have been delivered annually since 1905, the latest having been that of Professor G. C. Bourne on the 2nd December, 1909. The subject chosen by Professor Bourne was a re-examination of the questions at issue between Spencer and Weismann as to the nature of germ-cells; and the lecture was entitled "Herbert Spencer and Animal Evolution." It was published by the Clarendon Press in February.

In the course of some introductory remarks Professor Bourne revived and endorsed certain misrepresentations concerning Spencer, and in addition made other attacks on Spencer's methods. As it is impossible to answer these charges satisfactorily without referring to several volumes of Spencer's works it has been thought that the brief statement of the facts contained in this pamphlet may be useful.

In "The Life and Letters of Herbert Spencer" Dr. Duncan mentions that "With all his disregard for public opinion as far as concerned his philosophical doctrines . . . he was extremely sensitive to criticism of his character." Dr. Duncan also says:—"He was sensitive to anything in the way of misrepresentation and always took action at once, saying he kept in mind the proverb, 'Give a start to a lie and you can never overtake it.'" In so far as his own words afford a direct answer to Professor Bourne's attacks it is felt that Spencer would have desired that they should be cited; and excerpts have accordingly been made at what might otherwise appear inordinate

length. In a matter affecting Spencer's reputation, however, it would seem presumptuous either to omit or to attempt to improve an answer or explanation which he deemed sufficient.

In a chapter in "Facts and Comments," Spencer details and comments upon some of his personal experiences of mis-statements. For instance: After demonstrating by reference to the division of the *System of Synthetic Philosophy*, entitled "The Unknowable," and by quotations from the "Principles of Psychology," that there was no foundation for the ascription of materialism made against him by the late Professor Jowett, Spencer says:—

"But perhaps he did not know of these passages," some defender will say. "I am not aware that one who condemns an author's opinions is excused because he does not know what those opinions are; rather his ignorance adds to the gravity of his offence. But the excuse, bad though it is, is unavailing, for Professor Jowett had in his hands the works containing these passages. . . . In the presence of the quotations which I have given these statements will be thought incredible."

In one respect the above-mentioned case of injurious misrepresentation by Professor Jowett is, it will be seen, parallel to the case of the misrepresentations endorsed by Professor Bourne. Like Professor Jowett, Professor Bourne held in his hands the work containing Spencer's refutation of the charges he made.

A. W. T.

Witton, Norwich,
June, 1910.

Herbert Spencer, Hooker, and Huxley.

Not content with devoting The Herbert Spencer Lecture for 1909 to an unfavourable criticism of Spencer's hypothesis in connection with the proclivity towards a specific structural arrangement possessed by the germs of organisms (unfavourable in the double sense of being antagonistic whilst ignoring Spencer's arguments) the Lecturer, Professor G. C. Bourne, thought fit in addition to make certain disparaging statements concerning Spencer. Thus, in introducing his subject, Professor Bourne said :—

“ I have been speaking of Herbert Spencer as a biologist ; to most persons he appears as a philosopher who got his biological knowledge at second-hand and wove the discoveries and opinions of other biologists into the fabric of his philosophical system. He has been accused of borrowing his zoology from Huxley, his botany from Hooker. It cannot be denied that there is a grain of truth in this accusation.”

It may, perhaps, be doubted whether such statements call for more than passing notice. Whilst, however, he stated that Spencer “ has been accused of borrowing his zoology from Huxley, his botany from Hooker,” the Lecturer did not mention the fact that the Preface to Vol. I. of “ The Principles of Biology ” contains an explanation by Spencer of this “ gross misrepresentation ” as he (Spencer) called it ; nor did he state what was the actual fact as to the extent of the aid rendered to Spencer by Professor Huxley and Sir Joseph Hooker. It must be remembered, too, that Professor Bourne was delivering, at the University of Oxford, the Lecture founded to commemorate Spencer's name — the occasion being obviously one to which the maxim “ *De mortuis nil nisi verum* ” had a peculiar fitness. It would, therefore, be a natural inference that the Lecturer would minimise rather than exaggerate anything he mentioned which would detract from the value of Spencer's work. Besides, as a Professor of Zoology, dealing with a biological subject, his statements would, under the circumstances, appear to have a certain warrant of authority. Again, the subject of the lecture involved quotations from Spencer's “ Principles of

Biology"; and the omission by the Lecturer to make any reference to Spencer's refutation of the "gross misrepresentations" would naturally convey the impression that neither in "The Principles of Biology" nor elsewhere in his writings, was there any refutation by Spencer. It appears, therefore, that apart altogether from the question of their intrinsic importance, the circumstances invest Professor Bourne's statements with a certain extrinsic importance, in view of which they cannot be allowed to pass unchallenged. Such statements, too, are likely to be repeated in more definite form, and to be exaggerated.

In view of the character of the "gross misrepresentation" revived and endorsed by Professor Bourne, and in view of the fact that Spencer specifically dealt with perversions of a similar character, it appears that the most satisfactory way of dealing with Professor Bourne's statements will be, first of all, to quote what Spencer himself says in reference to this particular class of misrepresentation.

The Preface to Vol. I. of "The Principles of Biology" describes the aim of the work, and contains the following acknowledgment by Spencer of his indebtedness to Professor Huxley and Sir Joseph Hooker.

"For aid in executing it I owe many thanks to Professor Huxley and Dr. Hooker. They have supplied me with information where my own was deficient,* and in looking through the proof sheets have pointed out errors of detail into which I had fallen. By having kindly rendered me this valuable assistance they must not, however, be held committed to any of the enunciated doctrines that are not among the recognised truths of Biology."

The asterisk inserted by Spencer at the word "deficient" refers the reader to the following foot-note dated December 28th, 1874.

* "Gross misrepresentations of this statement which have been from time to time made oblige me, much against my will, to add here an explanation of it. The last of these perversions uttered in a lecture delivered at Belfast by the Rev. Professor Watts, D.D., is reported in the *Belfast Witness* of December 18th, 1874; just while a third impression of this work is being printed from the plates. The report commences as follows:—

"'Dr. Watts, after showing that on his own confession Spencer was indebted for his facts to Huxley and Hooker who,' &c., &c.

"Wishing in this, as in other cases, to acknowledge indebtedness when conscious of it, I introduced the words referred to in recognition of the fact that I had repeatedly questioned the two distinguished specialists named, on matters beyond my knowledge, which were not dealt with in the books at my command. Forgetting the habits of antagonists and especially theological antagonists, it never occurred to me that my expression of thanks to my friends for 'information where my own was deficient' would be turned into the sweeping statement that I was indebted to them for my facts.

"Had Professor Watts looked at the preface to the second volume (the two having been published separately, as the prefaces imply), he would have seen a second expression of my indebtedness 'for their valuable criticisms, and for the trouble they have taken in *checking* the numerous statements of fact on which the arguments proceed,' no further indebtedness being named. A moment's comparison of the two volumes in respect of their accumulations of facts, would have shown him what kind of warrant there was for his interpretation.

"Doubtless, the Rev. Professor was prompted to make this assertion by the desire to discredit the work he was attacking, and having so good an end in view, thought it needless to be particular about the means. In the art of dealing with the language of opponents, Dr. Watts might give lessons to Monsignor Capel and Archbishop Manning."

The italics, in which the word "*checking*" is printed, are Spencer's.

A comparison of the two volumes of "The Principles of Biology" in respect of their accumulation of facts, as suggested by Spencer, and made superficially (which Spencer indicates would be sufficient), discloses the fact that Vol. I. contains only one diagram—that in respect of an original classification of the animal kingdom—whereas Vol. II. contains 296 diagrams, without including those in the Appendices. On a cursory perusal of the pages, too, it is evident that the second volume contains a much greater accumulation of facts than the first volume. The second volume contains references to original observations and investigations on one hundred pages (more or less). The first volume refers to original observations on but relatively few pages. The difference in the two volumes between the number of the diagrams and the number of original observations recorded, may, doubtless, be taken to give a not very inaccurate idea of the ratio of the

accumulation of facts in the two volumes respectively. It will be particularly noted, therefore, that, notwithstanding the immensely greater accumulation of facts in the second volume, Spencer acknowledges his indebtedness to Professor Huxley and Sir Joseph Hooker in that volume "for their valuable criticisms, and for the trouble they have taken in *checking* the numerous statements of fact on which the arguments proceed," and adds, in the above quoted note, "no further indebtedness being named."

Without in any way suggesting that the above mentioned explanation by Spencer is not all-sufficient to satisfactorily dispose of Professor Bourne's endorsement of the statement that Spencer "has been accused of borrowing his zoology from Huxley, his botany from Hooker," it will not be irrelevant to mention the significant fact that *Spencer was an exponent of Organic Evolution before he had made the acquaintance of either Professor Huxley or Sir Joseph Hooker*. Spencer did not make Huxley's acquaintance until the year 1852—the first step being, says Dr. Duncan in "The Life and Letters of Herbert Spencer" a note "dated September 25, 1852." With Sir Joseph Hooker no intimacy existed until some years later, as in October, 1857, Sir Joseph Hooker ascribed the essay published by Spencer in that month entitled "The Ultimate Laws of Physiology" to Huxley. In March, 1852 (six months before he had met Huxley), Spencer's essay "The Development Hypothesis" was published. In April, 1852, Spencer published the essay "A Theory of Population." In both these essays Spencer, to a considerable extent, anticipated Darwin—a fact which illustrates the wide extent of his biological knowledge at the time. He says in the Autobiography that in November, 1858, "when I learnt the nature of Mr. Darwin's paper and learnt that Dr. Hooker accepted his interpretation, I sent copies of the volume to them and to a few others, because of the essay on the Development Hypothesis contained in it: the following is Mr. Darwin's acknowledgment:—No, it is not as follows: for on consideration I decide to omit it." The essay "A Theory of Population" is included in the Appendices to Vol. I. of "The Principles of Biology." Spencer there says that it was the germ of Part VI. of that work.

The wide extent of Spencer's botanical knowledge years before he knew Hooker is further and more definitely illustrated by an incident to which he refers in a letter to his father, dated September 22, 1851. "I mentioned to Lewes my notion about the law of vegetable development, and the carrying out of the idea in the examination of various plants added much to the interest of the walk." These observations ultimately resulted in the publication of an essay entitled "The Laws of Organic Form"—a title which Spencer says in the Autobiography "shows that the essay contains a further extension of the evolutionary views." Details of these observations are given in "The Principles of Biology." Spencer again refers to them in "The Filiation of Ideas." ("Life and Letters." Appendix B.)

Certain passages in "The Life of Professor Huxley" were the subject of a letter written by Spencer to Mr. Leonard Huxley, which appears in the Appendices to Vol. II. of the Autobiography. This letter is important as further defining the extent and nature of the "critical aid." It was as under:—

"5, Percival Terrace, Brighton,

"November 21, 1900.

"Dear Mr. Huxley,—On further reading your very interesting 'Life' of your father, I find some statements of personal concern which will cause much apprehension.

"Through inadvertence, passages on pages 333 of Vol. I. and 266 and 68 of Vol. II. convey the impression that the criticism of my proofs by your father extended to my writings at large; and a phrase of yours on page 133 of Vol. II. implies that you have yourself derived this impression. It is an erroneous one. Beyond 'First Principles' your father read in proof 'The Principles of Biology,' a biological essay and some chapters concerning the nervous system. There was peremptory need for expert criticism of these, and he very kindly gave me his; but I did not ask his critical aid when writing the seven volumes dealing with Sociology, Psychology, and Ethics, or the six volumes of my miscellaneous works, save the 15 pages of 'diabolical dialectics' (II., 185), and a chapter entitled 'Religious Retrospect and Prospect.' This is in a measure implied by my letter accompanying the proofs of the essay 'The Factors of Organic Evolution'—a letter in which I spoke of habitually submitting 'my biological writing to your (his) castigation' (2, 127), for had the practice been general, I evidently should not have limited the statement to biological writing.

"A word concerning the unpublished Autobiography. Reading of proofs by friends (your father being one) was to be a check on errors of taste. The parts your father saw amounted to about a third.

"When saying, à propos of his rôle of 'devil's advocate' that 'there is no telling how many brilliant speculations I have been the means of choking in an embryonic state,' your father was venting one of his facetious exaggerations. A comparison between the original MSS. and the printed books made by my secretary to whom I dictate this letter, shows that in the three volumes above-named, there are four passages of a speculative kind in the MS. which have disappeared from the printed text. (Let me add that of the two omitted from 'The Principles of Biology,' one concerned the derivation of the vertebrate type from the ascidian type—a speculation which not long after received support from the discoveries of Kowalewsky. I afterwards gave it a place in Appendix D of Vol. II.)

"As shown by a letter you have partly quoted, I have expressed my grateful sense of your father's 'invaluable critical aid,' but, naturally, I do not wish this to be understood as having been far greater than it was.

"Whatever changes you may make in future editions for the purpose of preventing misapprehensions, cannot of course be known to readers of the current edition. Yet I am not content that they should remain in error. What should be done ?

"Truly yours,

"HERBERT SPENCER."

In response to this letter Mr. Leonard Huxley published an explanatory letter in *The Athenæum* for December 8, 1900, in which he confirmed the letter he had received from Spencer.

Two chapters of Spencer's Autobiography are devoted to "The Principles of Biology," and further information will there be found as to reasons for the aid rendered by Huxley and Hooker. Referring to the issue of Vol. I. of "The Principles of Biology," Spencer, for instance, says :—

"Something by way of apology for venturing to deal with so vast and so difficult a subject seems called for—a subject too vast for any man fully to acquaint himself with as a whole—so vast that even one of its two great divisions is more than a diligent student can master—so vast that even a sub-division furnishes matter for investigation sufficient to occupy a life. . . .

"In my own case the presumption was diminished by the consciousness that friends who sympathised with my aims, and whose competence in their respective departments was beyond question, were prepared to aid me by their

criticisms. Professor Huxley kindly agreed to read through my proofs for the purpose of checking statements of zoological facts; while Dr. (now Sir Joseph) Hooker did the like for the botanical statements."

Reference may here not inappropriately be made to a striking passage in "The Life of Professor Huxley." Mr. Leonard Huxley says of his father:—

"In his biological studies he would take at second hand nothing for which he vouched in his teaching; so careful, indeed, was he in giving nothing at second hand, that one of his scientific friends reproached him with the waste of time upon unnecessary scientific work to which competent investigators had already given the stamp of their authority. 'Poor ———,' was his comment afterwards; 'if that is his own practice, his work will never live.'"

"The Life of Professor Huxley" contains evidence of remarkable praise accorded by Professor Huxley to Spencer's work; and to Spencer, it is mentioned, he once wrote:—

"But inadequately as I may ordinarily show it, you will (knowing that I am tolerably candid) believe me when I say that there is no one whose judgment on all subjects I so much respect, or whose friendship I so highly value."

With the above statement of facts the question of the extent and nature of the aid rendered to Spencer by Professor Huxley and Sir Joseph Hooker will be left; not, of course, with absolute satisfaction, justice to Spencer seeming to demand that something more should be said. In view of Spencer's character, and in view of his works, it is obvious that argument of irresistible force is available; and that the same conclusion as that to which a bare statement of the facts leads, would be inevitable from evidence of a different kind, equally indisputable. The argument, however, is unnecessary; and in honour to Spencer it appears preferable to omit it.

II.

Original Biological Investigations.

Professor Bourne also made the following assertion concerning Spencer:—

"The author of the 'Synthetic Philosophy' could not have completed his task—he could scarcely have begun it—if he had attempted to undertake the laborious and time-consuming labours of an original investigator of natural

phenomena, or even if he had attempted to verify by personal experience any considerable part of the evidence collected and handled in the two volumes of his 'Principles of Biology.' "

The statement that Spencer was not an original investigator of natural phenomena is, of course, a gross misrepresentation of fact ; and it is in addition obviously an allegation which, if true, would seriously affect the value of Spencer's biological work. There is, however, no difficulty in showing that the allegation is without foundation. "The Principles of Biology" (the book from which the Lecturer makes quotations) contains evidence of very extensive original investigation—very numerous investigations being specifically referred to by Spencer as investigations made by himself.

Parenthetically, reference may here not inappropriately be made to a letter written by Spencer to Sir Leslie Stephen. The letter was dated June 28, 1899. In it Spencer says :—

"All along I have looked at things through my own eyes, and not through the eyes of others. I believe that it is in some measure because I have gone direct to Nature, and have escaped the warping influences of traditional beliefs, that I have reached the views I have reached. . . . My own course—not intentionally pursued, but spontaneously pursued—may be characterized as little reading and much thinking, and thinking about facts learned at first hand."

Again in the Autobiography, Spencer says :—

"Never stopping to ask what has been thought about this or that matter, I have usually gone direct to the facts as presented in Nature, and drawn inferences afresh from them."

Were confirmation needed of the character of Spencer's methods which, of course, is not the case, it would be found in the esteem in which Spencer was held by Professor Huxley and other great men.

Why did Spencer omit from his Autobiography Darwin's letter acknowledging the receipt of the volume of essays containing the essay entitled "The Development Hypothesis ? " Undoubtedly because he regarded Darwin's acknowledgment as one of the highest compliments ever paid him—both in substance and in form.

It will be remembered that at the time when, for

want of funds, Spencer was contemplating the discontinuance of "The Principles of Biology," John Stuart Mill offered to guarantee Spencer's publisher against loss, and to co-operate with Spencer—a proposal between authors which Spencer refers to as having been "rarely, if ever, paralleled." A moment's reflection will show what probability there was that John Stuart Mill would make a proposal of this kind to an author who "got his biological knowledge at second hand."

Returning from this digression to the contemplation of the nature and extent of Spencer's original investigations, the following difficulty arises: How is it possible to convey an adequate idea of the superabundance of evidence of original investigation and original verification of facts contained in "The Principles of Biology." A cursory perusal discloses the record of observations and investigations by Spencer on upwards of a hundred pages—and this notwithstanding that Spencer's aim was to state general truths and not special truths. It appears impossible to do justice to the subject in the space available. The only satisfactory course is a perusal of "The Principles of Biology." The careless and inexcusable character of Professor Bourne's statement that Spencer was not an original investigator of natural phenomena will then soon become evident—particularly in Vol. II., which contains the greater accumulation of facts.

A remarkable instance of the character of some of Spencer's investigations will, however, be found in the fact that on March 1, 1866, he read a paper before the Linnean Society, in which he described certain experiments; and in the fact that two years later Sir Joseph Hooker gave Spencer's observations in his Presidential Address to the British Association as an "instance of successful experiment in Physiological Botany." In doing so Sir Joseph Hooker said: "It is an example of what may be done by an acute observer and experimentalist versed in Physics and Chemistry, but above all, thoroughly instructed in scientific methods."

It may be noted that "The Principles of Biology" contains an original classification of the animal kingdom, and 150 original biological diagrams. Details

of the experiments given by Sir Joseph Hooker will be found in the Appendices to "The Principles of Biology." The eight original diagrams contained in the paper read to the Linnean Society will be seen to be remarkable, both for their elaborate character and for the perfection of their execution. The paper extended to the length of about 15,000 (fifteen thousand) words and described many different kinds of experiments. Upwards of thirty different genera or species of plants formed the subject of these particular investigations.

Conclusion.

It would obviously be a matter for little surprise, in view of the mis-statements made by Professor Bourne, to find that to a great extent he does not appear to be cognizant with Spencer's fundamental biological principles—indeed, the most charitable explanation of his statements which it seems possible to suggest, is, that he was not so; although he made quotations from "The Principles of Biology." Without discussing the particular subject of the lecture, one or two instances of the complete misrepresentation of Spencer's doctrines are given as they will help to enable a correct value to be put on Professor Bourne's assertions. Thus Professor Bourne says (and the statement forms a separate paragraph in the lecture) :—

"To Herbert Spencer the development of the heterogeneous adult out of the homogeneous germ cell presented no great difficulty."

Professor Bourne ignores the following passages in "The Principles of Biology," which, it will be seen, are absolutely inconsistent with his interpretation :—

"At last, then, we are obliged to admit that the actual organizing process transcends conception. It is not enough to say that we cannot know it; we must say that we cannot even conceive it. And this is just the conclusion which might have been drawn before contemplating the facts. For if, as we saw in the chapter on 'The Dynamic Element in Life,' it is impossible for us to understand the nature of this element—if even the ordinary manifestations of it which a living body yields from moment to moment are at bottom incomprehensible; then, still more incomprehensible must be that astonishing manifestation of it which we have on the initiation and unfolding of a new organism.

“ Thus, all we can do is to find some way of symbolizing the process so as to enable us most conveniently to generalize its phenomena : and the only reason for adopting the hypothesis of physiological units or constitutional units is that it best serves this purpose.”

Again, Professor Bourne states :—

“ To explain the transition from non-living to living matter, he assumed the coming into existence of ‘ physiological units.’ ”

Professor Bourne makes no reference to the following passages in “ The Principles of Biology ” :—

“ We have no clue to the conditions under which inert proteids became so combined as to form active protoplasm.

“ That ‘ absolute commencement of organic life on the globe ’ which the reviewer says I ‘ cannot evade the admission of ’ I absolutely deny. The affirmation of universal evolution is in itself the negation of the absolute commencement of anything. Construed in terms of evolution, every kind of being is conceived as a product of modification wrought by insensible gradations on a pre-existing kind of being, and this holds as fully of the supposed ‘ commencement of organic life ’ as of all subsequent developments of organic life.

“ In brief, then, we are obliged to confess that Life in its essence cannot be conceived in physico-chemical terms.

“ What are we then to say—what are we to think ? Simply that in this direction, as in all other directions, our explanations finally bring us face to face with the inexplicable. The Ultimate Reality behind this manifestation, as behind all other manifestations, transcends conception.”

✧ Whilst it is preferable to make no comment on the methods of Professor Bourne, it is impossible not to enter a protest against the action, or inaction, of the Executive Authorities of the University. The primary responsibility for the appropriateness of the lecture to the occasion rests with the University.

Personal attacks upon Herbert Spencer appear, *primâ facie*, opposed to the intention of the Donor who gave £1,000 to found the Lectureship. Whatever may be the precise nature of the responsibility, legal or moral, which is imposed upon the University, the result of the lectureship, as above considered, seems to be little short of disgraceful.

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